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Trade liberalization and per capita income convergence: a difference-in-differences analysis

Matthew J. Slaughter*

*Dartmouth College and NBER, Dartmouth College, 309 Rockefeller Hall, Hanover, NH 03755,
USA*

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Abstract

In this article I analyze whether trade liberalization contributes to per capita income convergence across countries. The analysis focuses on four post-1945 multilateral trade liberalizations. To identify trade's effect on income dispersion, in each case I use a 'difference in differences' approach which compares the convergence pattern among the liberalizing countries before and after liberalization with the convergence pattern among control countries, chosen using a variety of methods, before and after liberalization. My main empirical result is I find no strong, systematic link between trade liberalization and convergence. In fact, much evidence suggests trade liberalization diverges incomes among liberalizers. © 2001 Elsevier Science B.V. All rights reserved.

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1. Introduction

Does trade liberalization cause per capita income convergence across countries? Trade theory offers an ambiguous answer: trade can either converge or diverge incomes. In this article I offer new empirical evidence on this question based on four important post-1945 trade liberalizations.

*Tel.: +1-603-646-2939; fax: +1-603-646-2122.

E-mail address: slaughter@dartmouth.edu (M.J. Slaughter).

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To identify trade's effect on income dispersion, in each of the four cases I use a 'difference in differences' methodology which compares the convergence pattern among the liberalizing countries before and after liberalization with the convergence pattern among control countries before and after liberalization. In each case I identify trade liberalization's role as the estimated difference in differences of convergence rates pre- and post-liberalization between the two groups of countries. As will be discussed, there is no obvious way to select control countries. So to gauge the robustness of results I try three different selection methods: using broad information on geography and/or income; using actual country-by-country data on income and trade flows; and simply picking countries randomly.

The central contribution of this article is its difference-in-differences methodology. The few existing empirical studies focusing on trade and income convergence attempt to identify trade's effect mainly through a single comparison (at most) of two groups of countries. All these studies argue that their single comparisons support the hypothesis that trade converges incomes. However, as I discuss below these single comparisons can actually be consistent with either convergence or divergence. The difference-in-differences approach is explicitly designed to overcome the potential ambiguities of the single-difference studies. As such, this approach tries to build on these important earlier studies. It is not without problems itself, but on balance it aims to sharpen the analysis of earlier work.

My main empirical result is I find no strong, systematic link between trade liberalization and convergence. This result comes from initial single-difference estimates and the core difference-in-difference estimates using two different convergence measures and a wide range of control groups. In fact, much of the evidence suggests that trade liberalization diverges incomes among liberalizers.

This article has five additional sections. Section 2 briefly summarizes some theory of trade and convergence. Section 3 reviews related empirical work and highlights some potential methodological limitations which this paper tries to improve upon. Section 4 outlines the difference-in-differences empirical strategy and discusses how I modify it to the current research question. Section 5 presents the data and empirical results. Section 6 summarizes the results and concludes with a discussion of my strategy in the broader context of future research ideas.

2. Trade and per capita income across countries: theory

Consider a country's per capita income (pcgdp) distributed to two factors of production, labor and capital.

$$\text{pcgdp} = \frac{\text{National Income}}{L} = \frac{w \times L + r \times K}{L} = w + r \times \frac{K}{L}$$

Here, L and K are the country's endowments of labor and capital and w and r are the respective national real factor prices for labor and capital. This set-up assumes

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that the total value of gross domestic output accrues to L and K and that for each factor there is one national market.

How can international trade affect convergence? Trade liberalization can influence each of the three basic determinants of national income as just written: factor prices, factor quantities, and production technology.¹

First, consider the factor prices w and r . The factor-price-equalization (FPE) theorem is usually defined as the set of endowment points among countries for which, given certain assumptions about technology and tastes, free trade entails not only equal goods prices across countries but also equal prices for nontradable factors as well.

As a statement about free-trade equilibria, however, the FPE theorem does not have clear dynamic predictions for the process of trade liberalization. Many studies have examined whether liberalization tends to bring factor prices closer together. The general answer is ‘maybe’: as with the FPE theorem, whether factor prices converge depends on cross-country tastes, technology, and endowments. Samuelson (1971) demonstrates that in the standard specific-factors framework freer trade very likely generates convergence. In contrast, Stiglitz (1970) finds the opposite when two countries have different rates of time preferences: freer trade tends to diverge factor prices. And Deardorff (1986) presents a two-country, two-factor, four-good Heckscher–Ohlin model in which freer trade converges product prices but actually diverges factor prices. Slaughter (1995) provides an empirical example: the construction of canals and railroads in antebellum United States converged interregional commodity prices but not interregional wages. Leamer and Levinsohn’s (1996) survey distinguishes the outcome of FPE from the process of wage convergence, and notes that lower trade barriers do not necessarily converge factor prices.

Next, consider endowments of L and K . There is a long history of extending static Heckscher–Ohlin models to allow factor accumulation over time (see, for example, Findlay’s, 1984 survey). The basic result here is that trade liberalization has an ambiguous net effect on endowments across countries. In some ways liberalization converges them but in other ways it diverges them.

One important way that liberalization can converge endowments is by reducing perceived investment risk in poorer countries. Lane (1997) formalizes a model in which trade agreements signal reduced investment riskiness in countries—par-

¹This discussion expands upon a shorter discussion in Slaughter (1997). Many papers on convergence cannot analyze the role of international trade because they assume a ‘Solow world’ in which countries producing a single aggregate good exist independent of one another (e.g., Barro and Sala-i-Martin (1992)). By construction, international linkages such as trade, factor mobility, and technology transfer don’t play any role. Instead, convergence arises from per capita capital stock convergence. Identical production technologies and time preferences across countries ensure (assuming no exogenous technological progress) that all countries tend towards the same (K/L) and thus the same w and r as well. It immediately follows that all countries reach the same pcgdp level and convergence is complete.

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ticularly in poorer countries which tend to be more risky initially. Post-liberalization capital accumulates more rapidly in now-less-risky poorer countries, and all else equal this tends to converge income across countries by raising (K/L) toward richer-country levels.

In contrast, there are at least two important ways for trade liberalization to work in the opposite direction. One is through Stolper–Samuelson effects on capital rentals. Baldwin (1992) formalizes how liberalization can generate ‘dynamic’ gains from trade for a country relatively well endowed with capital by raising its r (through the usual Stolper–Samuelson effect) and thus accelerating investment. For a country relatively poorly endowed with capital, dynamic losses from trade can result as liberalization lowers r and thus slows investment. The cross-country implication of these dynamic adjustments is divergence of relative endowments: capital-rich countries invest more while capital-poor countries invest less.

Another way freer trade can diverge cross-country endowments is by inhibiting the onset of diminishing marginal returns to investment. In a closed economy capital accumulation slows because of investment’s diminishing marginal physical productivity. But with free trade, FPE implies that a country faces constant marginal returns to investment. In an open economy investment changes the national output mix as predicted by the Rybczynski theorem instead of reducing r . Trade dampens the usual closed-economy convergence mechanism. This means that richer countries do not lose their incentive to invest as soon as they would in autarky. As a result, endowments across countries need not converge. Ventura (1997) argues that this process explains South Korea’s ongoing growth despite its prolonged high rates of investment.

Finally, consider technology. If countries have different levels of technology which can somehow transfer across countries, trade might be an important medium through which technology actually flows. This might happen as countries reverse engineer their imports or through the interpersonal contacts that accompany trade. Trade liberalization can thus boost technology flows.

Whatever the exact mechanism, trade-mediated technology flows change countries’ real factor prices and thus per capita income. Given a country’s endowment of inputs, improved technology implies higher marginal physical productivities for factors and thus higher real prices for these factors (*assuming* fixed world product prices). To the extent that technology flows from advanced to less-advanced countries, trade liberalization helps raise factor prices in less-advanced countries up towards the factor prices in advanced countries. Krugman (1979) presents a general-equilibrium model of this equalizing flow of technology.

If technology does not flow from advanced to less-advanced countries, however, then freer trade need not converge incomes across countries. Matsuyama (1996) formalizes a model in which freer trade alters international technology flows to generate divergence across countries. Here freer trade leads poorer countries to specialize in technologically-stagnant products because they lack the resources necessary to realize agglomeration economies of high-technology products.

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Meanwhile, richer countries grow even richer because they focus more on the high-technology products.

In summary, trade liberalization's many effects on cross-country levels of factor prices, factor quantities, and production technology have an unclear net effect on cross-country income levels.

3. Trade and per capita income across countries: empirical evidence

The existing evidence on trade and international income differences is mixed. There is some evidence that trade causes divergence and other evidence that it causes convergence. Moreover, the evidence on convergence may be ambiguous due to research design.²

Bernard and Jones (1996) provide some evidence that freer trade diverges income across countries. They document that cross-country productivity levels (measured as either labor or total-factor productivity) for individual manufacturing industries since 1970 have been either not converging or even diverging. In contrast, since 1970 cross-country productivity levels in services have been converging. To reconcile these facts Bernard and Jones hypothesize that international trade might be causing the divergence: 'in the tradable-goods sectors, comparative advantage leads to specialization, and to the extent that countries are producing different goods, there is no a priori reason to expect the technologies of production to be the same or to converge over time' (p. 1237).

In contrast with these results, Ben-David (1993, 1996) and Sachs and Warner (1995) present evidence linking trade to income convergence. These studies document historical episodes of income convergence across a group of countries that were relatively 'open' to each other (Sachs and Warner, 1995), that liberalized trade policy among each other (Ben-David, 1993), or that trade a lot with each other (Ben-David, 1996). The common conclusion of these papers is that international trade causes convergence. These papers contribute to the convergence literature by explicitly considering a role for trade. But their research design leaves their evidence somewhat open to interpretation.

Using several criteria addressing trade and finance policies and outcomes, Sachs and Warner classify each country in 1970 as either 'open' or 'closed.' From 1970 to 1989 only in the group of open countries did the poorer countries in 1970 tend to grow faster over the next 19 years. They conclude that '*the open economies display a strong tendency towards economic convergence ... We suggest that the most parsimonious reading of the evidence is that ... the convergence club is the club of economies linked together by international trade*' (p. 41).

A potential limitation of Sachs and Warner's methodology is they compare

²For a detailed survey of recent empirical studies on trade policy and growth, see Rodriguez and Rodrik (1999).

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closed and open economies only during the period in which the countries are classified as closed or open. They do not examine whether the behavior of each group might be changed over time because of being closed or open. Suppose that from 1950 through 1969 the subsequently open group had been converging even more rapidly than it did after 1970, while the subsequently closed group had been diverging even more rapidly than it did after 1970. Then one might conclude that openness slowed convergence, not accelerated it.³

Ben-David (1996) finds that from 1960 to 1989, groups of relatively wealthy countries which traded significantly among each other tended to display significant per capita income convergence relative to the convergence patterns of randomly grouped countries. He concludes that ‘These findings would appear to corroborate the intuition of Heckscher and Ohlin that trade does indeed play an equalizing role’ (p. 294).

By separating countries solely based on trade flows, Ben-David is more likely than Sachs and Warner to identify trade’s role in income convergence. But like Sachs and Warner, he compares two groups of countries (trading partners and random partners) only during the period in which the trading groups actually trade extensively. There is no effort to control for the groups’ convergence patterns during some earlier period. Again, ignoring this period might miss important information.

Finally, Ben-David (1993) analyzes five episodes of post-1945 trade liberalization, and finds that per capita income dispersion among liberalizing countries generally shrank after liberalization started. He writes that ‘This paper provides evidence that movement toward free trade may actually . . . [be] leading to a reduction in income disparity across countries . . . The [FPE] theorem provides a framework for relating trade’s impact on income convergence’ (p. 653).

Ben-David’s primary evidence that liberalization causes convergence is that during liberalizations, countries converged. He does make some comparisons of post-liberalization convergence either to the pre-liberalization experience of liberalizers or to a set of control countries. But these comparisons give only limited information. In Table 1 (p. 667) and Table 3 (p. 674), for various time periods and various country groups he reports estimated rates of income convergence. But he never explicitly matches groups during similar time periods and/or similar number of members. And he never formally tests for any differences among convergence rates, either within time periods across groups or within a

³A second issue regarding Sachs and Warner is their measurement of openness. They classify a country as either closed or open using five different criteria: non-tariff barrier coverage; tariff rates; black-market premia on exchange rates; overall economic system-socialist or capitalist; and the extent of government intervention in the export sector. A country is closed if it ‘fails’ any one of these five criteria. Because their openness measure combines many criteria it is difficult to interpret their results in terms of trade-related factors only. Rodriguez and Rodrik (1999) examine this issue in close detail.

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group across time periods.⁴ The lack of structural comparisons makes the results somewhat difficult to interpret.

In summary, Sachs and Warner and Ben-David have been among the first to test carefully for trade's effect on income convergence. As such, these studies are important contributions. However, these studies identify trade's effect mainly through a single comparison (at most) of two groups of countries. This use of just a single difference leaves the evidence somewhat open to interpretation. In what follows I aim to build on these studies by identifying trade liberalization's effect on convergence by using a difference-in-differences estimation strategy.⁵

4. Empirical strategy: difference-in-differences estimation

4.1. The general difference-in-differences methodology

The basic intuition of the difference-in-differences approach is that to study the impact of some 'treatment,' one compares the performance of the treatment group pre- and post-treatment relative to the performance of some control group pre- and post-treatment. In principle, the control group shows what would have happened to the treatment group in the absence of any treatment. Applied to the issue of trade liberalization's effect on income convergence, this approach suggests that one compare the convergence pattern among liberalizing countries pre- and post-liberalization with the convergence pattern among control countries pre- and post-liberalization. Trade liberalization's role is identified as the estimated difference in differences of convergence rates pre- and post-liberalization between the two groups of countries. If liberalization causes convergence (divergence), then the movement to freer trade should accelerate convergence (divergence) relative to what the rate would be absent liberalization.

To develop this empirical strategy I first present the typical difference-in-

⁴In the text discussing Tables 1 and 3 Ben-David does report whether each estimated convergence rate is significantly different from zero. But this information alone is not sufficient to determine whether a difference between two estimated convergence rates is statistically different from zero or whether a difference-in-differences among four estimated convergence rates is statistically different from zero. For example, in Table 1 EEC convergence was not statistically different from zero 1900–1933 but was statistically different from zero during liberalization. But it does not follow from this that the two convergence rates differ from each other: if the 1900–1933 convergence rate is estimated sufficiently imprecisely then one cannot reject the hypothesis that this rate is the same as the liberalization rate.

⁵Some related studies include Mokhtari and Rassekh (1989) and Davis (1992), who test for a link between wage convergence across OECD countries and trade flows. Also, Eaton and Kortum (1995) find that growth in many OECD countries is accounted for by ideas generated in foreign countries while Coe and Helpman (1995) and Coe et al. (1997) present evidence that trade mediates the flow of these ideas and their impact on total factor productivity. However, none of these studies focuses directly on trade and income convergence as do the papers discussed in this section.

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differences approach, following the detailed discussion in Meyer (1994). I then discuss how I modify this typical approach to cases of trade liberalization, with particular focus on the issues of defining treatment and determining control groups.

Suppose a group of economic agents has some treatment applied at a single point in time, and suppose further that some outcome for these agents can be observed both before and after treatment application. Then one could try to estimate the treatment effect with the following regression:

$$y_{it} = \alpha + \beta d_t + e_{it}, \quad (1)$$

where y_{it} is the outcome for agent i ($i=1, \dots, N$) at time t ($t=0$ or 1), d_t is a dichotomous variable equal to one if $t=1$ and zero if $t=0$, and e_{it} is an error term (whose variance varies by t). β identifies the causal effect of treatment under the identifying assumption that $E[e_{it}|d_t] = 0$, i.e., that without treatment all agents would be comparable over time (such that without treatment $\beta=0$). β can be determined by estimating Eq. (1) or simply by calculating the single difference of the change in mean outcomes before and after treatment (i.e., the average outcome at $t=0$ minus the average outcome at $t=1$). For example, in Card and Krueger (1994) the treatment was an increase in New Jersey's statutory minimum wage on April 1, 1992. The outcome of interest was employment in New Jersey fast-food restaurants, so to implement (1) one might measure employment for a sample of N New Jersey fast-food restaurants at $t=0$ (February 1992 in this study) and then again at $t=1$ (November 1992). If the identifying assumption were valid, then from (1) β would identify the employment effect of the minimum-wage change.

A problem with this single-difference approach is potential violation of the identifying assumption. Between $t=0$ and $t=1$ many forces other than treatment might affect the outcome of interest. For example, New Jersey fast-food employment might change because of the business cycle or shifts in consumer tastes. The essence of the difference-in-differences approach is to try to account for these other forces by also examining the outcomes for a control group that does not receive the treatment but that presumably is affected by these other forces. This suggests

$$y_{it}^j = \alpha + \alpha_1 d_t + \alpha^1 d^j + \beta d_t^j + e_{it}^j, \quad (2)$$

where now j indexes the two groups with $j=1$ for the treatment group and $j=0$ for the control group; d^j is a dichotomous variable equal to one if $j=1$ and zero if $j=0$; and d_t^j is a dichotomous variable equal to one if both $j=1$ and $t=1$, and zero otherwise. β is again the key coefficient which identifies the causal effect of treatment. It is obtained by estimating (2) or simply by calculating the ‘difference in differences’ equal to the change in mean outcomes for the treatment group minus the change in mean outcomes for the control group. The parameter α_1 captures how both groups are affected over time by any non-treatment forces,

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while the parameter α^1 captures any time-invariant difference in outcomes between the treatment and control groups. Similar to in (1), the key identifying assumption in Eq. (2) is that $E[e_{it}^j | d_t^j] = 0$, i.e., that $\beta = 0$ in the absence of treatment. This assumption ‘is most plausible when the untreated comparison group is very similar to the treatment group’ (Meyer, p. 18). Card and Krueger use fast-food restaurants in eastern Pennsylvania as the control group to account for non-treatment forces such as seasonality: ‘since seasonal patterns of employment are similar in New Jersey and eastern Pennsylvania . . . our comparative methodology effectively differences out any seasonal employment effects’ (p. 773).⁶

4.2. The difference-in-differences methodology applied to trade liberalization

The task now is how to apply the difference-in-differences approach to the issue of trade liberalization and per capita income convergence. A first issue to consider is the treatment. In typical difference-in-differences studies in economics (mostly public finance and labor economics) the treatment is a one-time change in government policy applied equally to all members of the treatment group. The equal application allows identification of the treatment and control groups. And the one-time nature of the change makes it easy to select specific pre- and post-treatment points in time, where by the latter it is assumed that the full treatment effect has been realized.

Real-world trade liberalization tends to differ from the typical treatment in three important ways. First, lots of trade liberalization (or restriction) is undertaken unilaterally or multilaterally but to different extents across countries. This suggests that a difference-in-difference analysis of trade liberalization requires cases where all trading partners liberalized to the same extent following the same timetable. Second, as discussed in Section 2, many convergence effects of liberalization are likely to appear at the same time as barrier reduction. For example, as barriers fall product prices change, which in turn may trigger responses including FPC and altered investment. This suggests that the post-treatment period should be defined as the entire time period of liberalization. Third, historical trade liberalizations usually have not been one-time events but rather have been implemented gradually over multiple years. In these cases the treatment effect, convergence post-treatment, should be looked for at multiple points in time rather than just at a single point.

In light of these issues, for this study I define the treatment to be a sizable trade liberalization implemented equally by all participants with clear starting and stopping dates. The clear dates identify the span of the post-treatment period during which effects will be looked for. I require sizable liberalizations assuming they are more likely to affect income convergence than minor liberalizations. For

⁶Implementing (2), Card and Krueger find, somewhat surprisingly, that relative to changes in Pennsylvania, New Jersey fast-food employment rose, not fell, after the minimum-wage increase.

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symmetry, I define the pre-treatment period to be as long as the liberalization period, data permitting. As discussed below, in two of the four cases the data do not permit longer pre-treatment periods; in all cases, the basic results are robust to shortening or (when possible) lengthening the pre-treatment period by one or two years.

The outcome of interest is some measure of per capita income dispersion. For each case I measure per capita income dispersion among the treatment group and among the control group two different ways, both of which have are commonly used (e.g., Ben-David, 1993, 1996). The first is the standard deviation of the natural log of per capita income among a group of countries.⁷ For this dispersion measure, the analog of Eq. (1) which I estimate is given by Eq. (3):

$$\sigma_{rt} = \alpha_1 + \alpha_2(d_r) + \beta_1(t) + \beta_2(t)(d_r) + e_{rt}, \quad (3)$$

where t indexes time in years running from $t = 1$ until $t = T$; r indexes the two equal-length regimes of interest: $r=0$ for the pre-liberalization regime running from $t=1$ until $t = (T/2)$ and $r=1$ for the post-liberalization regime running from $t = (1 + T/2)$ until $t = T$; d_r is a dichotomous variable equal to one if $r=1$ and zero if $r=0$, and e_{rt} is an error term (whose variance varies by r). The regressand σ_{rt} is the standard deviation of the natural log of per capita income among the liberalizing countries for regime r at time t . Eq. (3) is basically a spline regression where both regimes are allowed to have different intercepts and parameters on time. The rate of change in income dispersion pre-liberalization is given by β_1 , and post-liberalization it is given by $(\beta_1 + \beta_2)$. In both regimes income convergence (divergence) is indicated by negative (positive) slope coefficients. The single difference β_2 indicates whether post-liberalization convergence differs from that pre-liberalization. This single-difference coefficient is analogous to the coefficient β in Eq. (1).

For the difference-in-differences specification, let the superscript j indicate country group, with $j=1$ the liberalizing group and $j=0$ some control group. Then the analog to Eq. (2) which I estimate is given by Eq. (4),

$$\begin{aligned} \sigma_{rt}^j = & \alpha_1 + \alpha_2(d_r) + \alpha_3(d_r^j) + \alpha_4(d_r^j) + \beta_1(t) + \beta_2(t)(d_r) + \beta_3(t)(d_r^j) \\ & + \beta_4(t)(d_r^j) + e_{rt}^j, \end{aligned} \quad (4)$$

where the dichotomous variable d_r^j indicates country group; the dichotomous variable d_r^j equals one if both $j=1$ and $r=1$ and zero otherwise; and e_{rt}^j is an error term (whose variance varies by both j and r). For each of the four country-group/

⁷A common test for convergence is to regress a cross-section of countries' annualized income growth rates on their initial income levels: a negative coefficient indicates convergence. In theory this measure and that in the text need not imply each other even though in reality they often seem to coincide. In the terminology of Barro and Sala-i-Martin, the measure just described is 'beta' convergence while that in the text 'sigma' convergence.

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regimes, Eq. (4) estimates a separate intercept term and convergence rate for income dispersion.

Country-group/regime	Intercept	Convergence rate
Liberalizing group pre-liberalization	α_1	β_1
Liberalizing group post-liberalization	$\alpha_1 + \alpha_2$	$\beta_1 + \beta_2$
Control group pre-liberalization	$\alpha_1 + \alpha_3$	$\beta_1 + \beta_3$
Control group post-liberalization	$\alpha_1 + \alpha_2 + \alpha_3 + \alpha_4$	$\beta_1 + \beta_2 + \beta_3 + \beta_4$

The effect of trade liberalization on income convergence can be obtained by calculating the ‘difference in differences’ of the estimated rates. The difference in convergence rates within the liberalizing group pre- and post-liberalization is given by β_2 . The similar difference in convergence rates within the control group is given by $(\beta_2 + \beta_4)$. Thus the difference in differences is given by $(\beta_2 + \beta_4) - (\beta_2) = \beta_4$. Assuming that the only treatment pre- and post-liberalization between the two groups is liberalization, β_4 identifies its effect. If trade liberalization tends to converge (diverge) incomes among the liberalizing countries then β_4 is positive (negative). Any cross-regime time effect on convergence common to both groups of countries is captured by β_2 , and any time-invariant differences in convergence between groups is captured by β_3 .⁸

My second measure of income dispersion captures how quickly each country’s income level is converging to the average income level of that country’s group. Following Ben-David (1993), let

$$(y_{it+1} - \bar{y}_{it+1}) = \gamma(y_{it} - \bar{y}_t), \quad (5)$$

where y_{it} is country i ’s log real income per worker in year t , \bar{y}_t is the arithmetic average of y_{it} , and γ is a parameter relating the average income gap from one year to the next. Then define $z_{it} = (y_{it} - \bar{y}_t)$ and $\Delta z_{it+1} = (z_{it+1} - z_{it})$, and Eq. (5) can be manipulated to obtain the following equation for estimation:

$$\Delta z_{it+1} = \delta(z_{it}) + u_{it}, \quad (6)$$

where $\delta < 0$ represents the rate of convergence of y_{it} to \bar{y}_t (with $\gamma - \delta = 1$). The

⁸For example, suppose that pre-liberalization neither group had any trend in income dispersion but that after liberalization the liberalizing group converged at a rate of -0.1 while the control group diverged at a rate of 0.1 . Then the difference within the liberalizing group equals $(-0.1) - (0) = -0.1$. Similarly, the difference within the control group equals $(0.1) - (0) = 0.1$. Then the difference in differences is given by $(0.1) - (-0.1) = 0.2 > 0$. Trade liberalization leading to convergence is captured by a positive b_4 . There is alternative way to calculate the difference in differences. First calculate the pre-liberalization difference in convergence rates between the two groups, b_3 . Then calculate the post-liberalization difference in convergence rates between the two groups, $(b_3 + b_4)$. Then the difference in differences is $(b_3 + b_4) - (b_3) = b_4$.

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larger (in absolute value) is δ_1 , the faster is the convergence. For the initial single-difference analysis, (6) becomes

$$\Delta z_{rit+1} = \delta_1(z_{rit}) + \delta_2(z_{rit})(d_r) + u_{rit}, \quad (7)$$

where r again indexes regime, d_r is defined as before, and u_{rit} is an error term (whose variance varies by r). Now the single difference δ_2 indicates whether post-liberalization convergence differs from that pre-liberalization. Like β_2 in Eq. (3), this single-difference coefficient is analogous to the coefficient β in Eq. (1). For the difference-in-differences specification, again let the superscript j indicate country group. This gives

$$\Delta z_{rit+1}^j = \delta_1(z_{rit}^j) + \delta_2(z_{rit}^j)(d_r) + \delta_3(z_{rit}^j)(d_r^j) + \delta_4(z_{rit}^j)(d_r^j) + u_{rit}^j, \quad (8)$$

where all variables are defined as before and u_{rit}^j is an error term (whose variance varies by both j and r). As in (4), the effect of trade liberalization on income convergence can be obtained by calculating the difference in differences of the estimated rates, equal to δ_4 here. If liberalization tends to converge (diverge) incomes among the liberalizing countries then δ_4 is positive (negative). Any cross-regime time effect on convergence common to both groups of countries is captured by δ_2 , and any time-invariant differences in convergence between groups is captured by δ_3 .⁹

In both Eqs. (4) and (8), the key identifying assumption is that the only difference pre- and post-liberalization between the two groups is trade liberalization. This is equivalent to saying that β_4 or δ_4 would be zero in the absence of liberalization. This identifying assumption means that all the unobservables not included in Eq. (4) have a special time-series structure in which there is no shock that affects the relative outcome of the two groups at the same time as the trade-liberalization shock. But for trade liberalization, there can be no shock affecting the relative outcome of the two groups pre- and post-liberalization.

This leads naturally to the question of how to select control groups. To answer this, Table 1 first lists the four liberalization groups analyzed in this paper: formation of the European Economic Community (EEC); formation of the European Free-Trade Area (EFTA); liberalization between the EEC and EFTA; and the Kennedy round of the General Agreement on Tariffs and Trade (GATT). The information on participating countries and both dates and depth of liberalization comes from Ben-David (1993) and United Nations (1968). I selected these cases because they all involved substantial barrier cuts and because the cuts were made equally by all members in a well-specified timetable. For each case I include all countries which actually implemented the stated barrier cuts in the stated

⁹As in Ben-David (1996), one could extend Eqs. (7) and (8) by using longer lags of z as additional regressors. I do not do this because, as shown below, two of my four cases (EEC and EFTA cases) do not have early enough data to calculate longer lags in the pre-liberalization regime.

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Table 1
Cases of trade liberalization

Case name	Liberalizing countries	Pre-liberalization period	Liberalization period	Tariff cuts within group	# of control countries
EEC	Benelux, France, Germany, Italy	1950 thru 1958	Jan. 1959 to July 1968	100%	54
EFTA	Austria, Denmark, Norway, Sweden, Switz., UK	1952 thru 1959	July 1960 to Jan. 1967	100%	54
EEC-EFTA	EEC plus EFTA	1958 thru 1967	July 1968 to Jan. 1977	100%	60
Kennedy Round	37 altogether ^a	1963 thru 1967	Jan. 1968 to Jan. 1972	50%	88

^a Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, Czechoslovakia, Denmark, Dominican Republic, Finland, France, Germany, Greece, Iceland, India, Israel, Italy, Jamaica, Japan, Korea, Luxemborg, Malawi, Netherlands, N. Zealand, Norway, Peru, Portugal, South Africa, Spain, Sweden, Switzerland, Trinidad and Tobago, Turkey, United Kingdom, United States, Yugoslavia.

Source: Ben-David (1993).

timetable. In each case, for defining the post-liberalization period starting years include the year of first reductions if the date was June or earlier; ending years are the calendar year of the final tariff cut regardless of its month. The pre-liberalization period I then define as covering the same number of years (data permitting, as the data start in 1950). For example, the EEC case's post-liberalization period was 1959 through 1968, so pre-liberalization was 1950 through 1958.

Control countries should be 'very similar to the treatment group' (Meyer, p. 18) but for the treatment. What this means in practice is not obvious. Accordingly, I select control countries three different ways. First, I define control groups by broad information on geography and/or income. For example, GATT's 37 liberalizers span a wide range of developed and developing countries all over the world. So I construct its control group to be all countries in the world not part of the Kennedy round. For continuity, in each of the other three cases I also construct a rest-of-the-world control group as all countries other than that case's liberalizers. These other three cases consist of European countries which were wealthy relative to most of the world. So for these three cases I also use as control groups all other European countries and all other original Organization of Economic Cooperation and Development (OECD) countries. Thus, by this first selection method the first three cases have three control groups each and the fourth case one. Also, all the EEC (EFTA) control groups also exclude the EFTA (EEC) countries because both sets of countries had very similar treatments at almost exactly the same time. Note that different control groups have different numbers of countries. Given the criterion of a particular control group, the number of countries is constrained only by data availability: for a country to be in that group there must be data on per

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per capita income for every year of the pre- and post-liberalization periods. This prevents changes in income dispersion due to countries entering and exiting the sample.

My second selection method uses actual data on per capita income and trade openness (exports plus imports as a share of GDP) to pick countries which look most similar to the liberalizing countries. Convergence patterns may depend on per capita income, e.g., convergence mechanisms may vary with overall development. And Ben-David (1996) links trade flows and convergence; perhaps the role of trade-mediated convergence mechanisms depends on the size of trade volumes.

For each case, for each liberalizing country during the pre-liberalization period I calculate average per capita income and average trade openness. Next, I calculate similar averages for all countries in each of that case's first set of control groups. From each of these control groups I then pick the one country which is most similar to the liberalizing country in question, where similarity is measured as follows. First rank countries based on smallest absolute difference in per capita incomes, then rank them the same way based on smallest absolute difference in trade openness, and then sum the two ranks and pick the one country with the closest overall rank. In cases where two liberalizing countries share the same most-similar control country, I use second-closest control countries (and beyond, when necessary) following the principle of maximizing overall closeness of the liberalizers as a group.¹⁰ Overall, this second selection method generates the same number of control groups per case as the first method, but here each control group has the same number of countries as that case's liberalizing group.

My third selection method simply picks control countries randomly from the rest of the world. Specifically, for each case I select randomly from all other countries in the world for which there is adequate data a number of countries equal to the number of liberalizers. Because I have no idea if any particular random group is appropriate, I repeat this exercise 1000 times for each case and report average estimation results.

Each method for selecting control groups picks ‘similar’ countries differently. The first uses broad information on geography and/or income; the second uses actual country-by-country data on income and trade flows; and the third simply picks countries randomly. Which mechanism is best depends on how well one thinks data on geography, income, and/or trade flows find countries satisfying the difference-in-differences identification assumption. It is true that during each case many other countries were also liberalizing trade. But references on recent trade agreements (e.g., Bhagwati and Panagariya, 1996) do not reveal any country groups which were liberalizing the same way as any of the treatment groups—e.g.,

¹⁰For example, suppose that liberalizer L1 is closest to potential control C1 with an overall rank of two and then next closest to potential control C2 with an overall rank of four. Liberalizer L2 is also closest to potential control C1 with an overall rank of two, but is next closest to potential control C3 with an overall rank of 20. I would then select C1 to be the control for L2 and C2 to be the control for L1, because C2 is ranked much closer to L1 than C3 is to L2.

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during the Kennedy Round the rest of the world was not also reducing tariffs by 50%. This suggests there are meaningful differences between treatment and control groups. For completeness I report results using all three mechanisms.

To summarize, my empirical plan is to estimate Eqs. (3), (4), (7), and (8) on each of the four cases listed in Table 1, where for Eqs. (4) and (8) all control groups just described are used. All equations are estimated using ordinary least squares with robust standard errors to allow for general heteroskedasticity in the error terms.

Before reporting the estimation results, it helps to relate this plan's key equations to earlier work. Both Sachs and Warner and Ben-David (1996) compare the convergence of a group of already open (Sachs and Warner) or already trading (Ben-David) countries with a control group of closed or randomly selected countries. In terms of Eq. (4), these studies contrast $(\beta_1 + \beta_2 + \beta_3 + \beta_4)$ with $(\beta_1 + \beta_2)$. This single difference $(\beta_3 + \beta_4)$ may suffer from not controlling for any differences between the two groups that may have predated the influence of openness/trade. In addition, these two studies do not have a clear policy treatment. Sachs and Warner measure openness using both policies and market outcomes; Ben-David's country groupings are based on endogenously determined trade flows. Ben-David (1993) is the study closest in method to this one. His analysis of trade-liberalization cases do have clear treatments. But much of his analysis looks at the performance of liberalizing countries post-liberalization, i.e., $(\beta_1 + \beta_2)$, or (somewhat informally) at this performance relative to the pre-liberalization performance, β_1 . These quantities need not bear any systematic relationship to β_4 .

5. Data and empirical results

5.1. Data

All data come from the Penn World Tables of Summers and Heston (1997). Both real income per capita and per worker are in this data set. I use income per worker because most convergence models assume all people work. Because there is a very high sample correlation between labor forces and populations, results are very similar using either measure.

5.2. Single-difference estimation results

As a way to visualize the regression results for the single-difference estimates, Fig. 1 plots σ_{rt}^j over time for each of the four cases listed in Table 1. If trade liberalization were converging incomes, then one might expect to see convergence accelerate after each trade agreement started. But none of the four cases displays dramatic acceleration. Indeed, in the EFTA and EEC-EFTA cases convergence appears to slow post-liberalization. The GATT case has convergence post-liberalization, but it mostly occurs in one year. Also, the range of income dispersion is

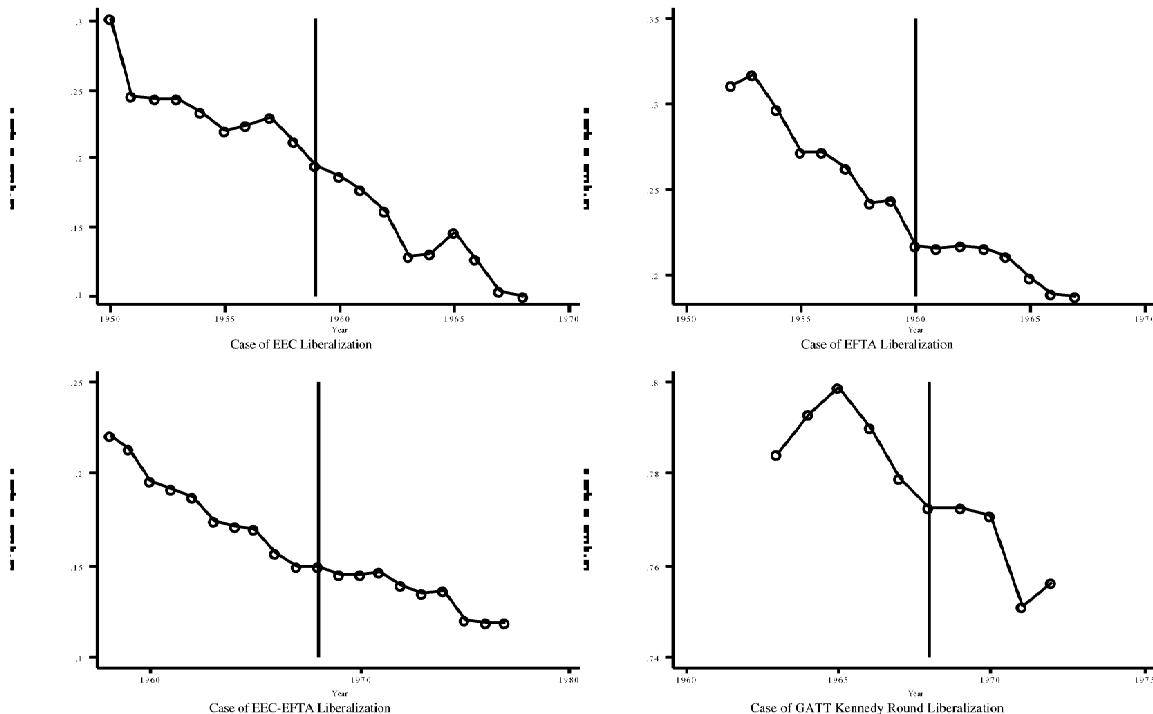


Fig. 1. Per capita income dispersion for the four cases, pre- and post-liberalization. In all panels the vertical axis measures per capita income dispersion among the liberalizing countries, measured as the standard deviation of the natural log of per capita incomes. In all panels the vertical line marks the onset of trade liberalization.

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Table 2

Differences in rates of per capita income convergence pre- vs. post-liberalization for liberalizing countries^a

Case name	Pre-liberalization convergence rate for liberalizers	Post-liberalization convergence rate for liberalizers	Difference in convergence rates	Number of observations
EEC	−0.008	−0.011	−0.003	19
Liberalization	(−2.888)**	(−17.550)***	(−1.006)	
EFTA	−0.011	−0.005	0.006	16
Liberalization	(−9.626)***	(−6.164)***	(4.620)***	
EEC-EFTA	−0.007	−0.003	0.004	20
Liberalization	(−17.192)***	(−9.194)***	(6.327)***	
Kennedy Round	−0.001	−0.005	−0.004	10
Liberalization	(−0.513)	(−2.224)*	(−1.254)	

^a These convergence rates are parameter estimates of the difference-in-differences regression specified in Eq. (3). Reading across, the columns report the following parameters: β_1 ; $(\beta_1 + \beta_2)$; β_2 . T -statistics are reported in parentheses for robust standard errors. ***, **, and * denote significance at the 99%, 95%, and 90% levels.

much narrower in this case than the other three, suggesting this case might not be as informative.

Table 2 presents the regression results for Eq. (3) for all four cases. The key coefficient estimate is β_2 , where a negative (positive) β_2 indicates that convergence accelerated (decelerated) with the onset of liberalization. In none of the four cases is convergence significantly faster after liberalization starts. In the EEC and GATT cases convergence accelerated, but not significantly. In the EFTA and EEC-EFTA cases convergence decelerated significantly. Table 3 presents the

Table 3

Differences in rates of per capita income convergence pre- vs. post-liberalization for liberalizing countries^a

Case name	Pre-liberalization convergence rate for liberalizers	Post-liberalization convergence rate for liberalizers	Difference in convergence rates	Number of observations
EEC	−0.059	−0.084	−0.025	108
Liberalization	(−2.448)**	(−3.807)***	(−0.762)	
EFTA	−0.037	−0.037	0.000	112
Liberalization	(−2.770)***	(3.028)***	(0.021)	
BEC-EFTA	−0.054	−0.034	0.020	260
Liberalization	(−4.822)***	(−2.509)**	(1.138)	
Kennedy Round	−0.004	−0.007	−0.003	370
Liberalization	(−0.761)	(−1.415)	(−0.394)	

^a These convergence rates are parameter estimates of the difference-in-differences regression specified in Eq. (7). Reading across, the columns report the following parameters: δ_1 ; $(\delta_1 + \delta_2)$; δ_2 . T -statistics are reported in parentheses for robust standard errors. ***, **, and * denote significance at the 99%, 95%, and 90% levels.

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analogous regression results for Eq. (7). Here the key coefficient estimate is δ_2 , with a negative (positive) δ_2 indicating that convergence accelerated (decelerated) with the onset of liberalization. The basic conclusion is the same as from Table 2. In none of the four cases is convergence significantly faster after liberalization starts: in all cases income levels were converging to group averages both before and after liberalization, but with no significant difference between the two regimes. On balance, the single-difference estimates from both convergence measures suggest that trade liberalization did not foster any significant, systematic convergence.¹¹

5.3. Difference-in-difference estimation results

As discussed in Section 4, these single-difference results might be masking the true effect of trade liberalization. For example, in each case perhaps post-liberalization there was rapid income divergence in the rest of the world. Then despite the single-difference evidence, liberalization may have fostered convergence among the liberalizers relative to the control group of the rest of the world. The difference-in-differences regressions in Eqs. (4) and (8) allow for this possibility.

Figs. 2 and 3 provide a visual preview of the difference-in-differences results using the EEC case as an example (for brevity, figures for the other three cases are omitted). Fig. 2 plots σ_{rt}^j over time for the EEC countries and for three possible control groups: the rest of the world, the rest of the OECD, and the rest of Europe. Fig. 3 plots EEC σ_{rt}^j along with three ‘ideal’ control groups constructed from data on both per capita income and trade openness: one group picked from the rest of the world, one from the OECD, and one from Europe (see Section 4 for details on the control groups). Recall from Fig. 1 and Table 1 that among the EEC countries convergence accelerated a bit post-liberation, but not significantly. Pairing this EEC difference with the respective differences for the control groups visualizes the difference in differences to be estimated.

In Fig. 2 no control group shows a dramatic change in convergence post-EEC liberalization. In particular, none shows a marked shift towards less convergence or greater divergence. This suggests that difference-in-difference estimates will not indicate that EEC liberalization caused convergence. In Fig. 3 both the OECD and world control groups appear to have sharp changes towards convergence post-EEC

¹¹The results in Tables 2 and 3 are generally robust to dropping a year from or (where data permit) adding a year to the start of the pre-liberalization period and/or the end of the post-liberalization period. The only ‘sensitive’ endpoint year was 1950 in the EEC case. As Fig. 1 shows, the EEC group converged substantially between 1950 and 1951. If this case starts in 1951, then both the single-difference estimates in Tables 2 and 3 become significant at the 5% level. However, starting this case in 1950 is preferable both because the additional year makes the pre-liberalization period closer in duration to the post-liberalization period and because there is no clear reason why the 1950 information should be ignored.

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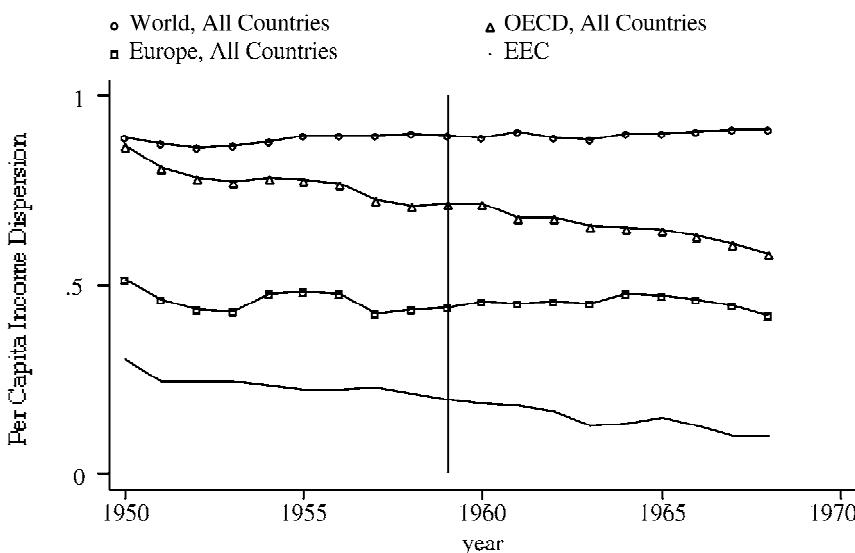


Fig. 2. Per capita income dispersion for the EEC and three possible control groups, pre- and post-liberalization. In all panels the vertical axis measures per capita income dispersion among the liberalizing countries, measured as the standard deviation of the natural log of per capita incomes. The vertical line marks the onset of EEC trade liberalization. See the text for a description of the construction of the control groups.

liberalization (initially slow convergence sped up for the OECD, and initially rapid divergence stopped for the world), suggesting that these difference-in-difference estimates will indicate EEC liberalization actually caused divergence. These ‘visual difference-in-differences’ also highlight that what matters for a control group is its outcome change relative to the treatment group’s outcome change, not its overall outcome relative to that of the treatment group. For example, in Fig. 2 there is no worldwide income convergence during the overall period—if anything there is moderate divergence.¹² But the relevant comparison is not overall worldwide no convergence versus overall EEC convergence: it is the change in convergence patterns across the two groups. Indeed, by construction the difference-in-differences approach accounts for these overall, time-invariant differences in outcomes between the two groups (coefficient β_3 in Eq. (4) and δ_3 in Eq. (8)).

Tables 4–7 present the difference-in-differences estimation results using all three selection methods for control groups, one case per table. Each row of each table reports the difference-in-differences parameter estimate from either Eq. (4)

¹²This unconditional divergence of worldwide incomes has recently been noted in the growth literature as a general pattern of the 20th century (e.g., Pritchett, 1997).

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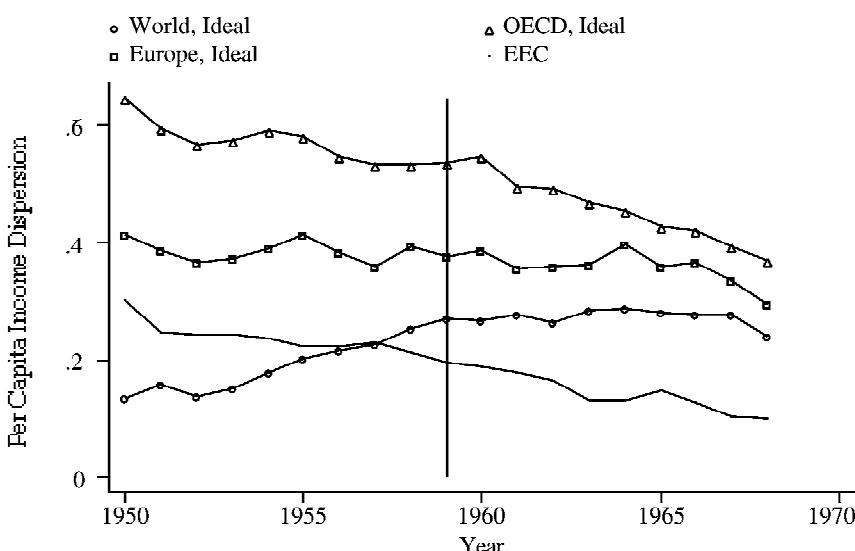


Fig. 3. Per capita income dispersion for the EEC and three possible control groups, pre- and post-liberalization. In all panels the vertical axis measures per capita income dispersion among the liberalizing countries, measured as the standard deviation of the natural log of per capita incomes. The vertical line marks the onset of EEC trade liberalization. See the text for a description of the construction of the control groups.

(coefficient β_4) or Eq. (8) (coefficient δ_4). Each row also reports the control group used, the number of countries in that control group, and the number of observations in the estimation equation. For the randomly chosen control groups I report the average parameter estimate for all 1000 trials and for just the trials with a significant parameter estimate. A positive (negative) estimate of β_4 or δ_4 indicates that trade liberalization converges (diverges) per capita incomes among the liberalizing countries.

The overall message of Tables 4–7 is that trade liberalization does not generate any significant, systematic convergence. For the EEC case in Table 4 the estimates are somewhat mixed. The all-country and randomly chosen control groups indicate insignificant convergence, while the ‘ideal’ data-chosen control groups mostly indicate divergence—significantly so in one case. In each of Tables 5 and 6, all estimates but one indicate divergence, significantly so in many cases. The GATT estimates in Table 7 show convergence, but only one is marginally significant.

In evaluating the evidence in these tables there are a few points of note. First, once in Table 5 and twice in Table 6 a data-chosen control group duplicates an all-country control group: this is because there simply weren’t enough countries in the all-country pools for the data to choose from. Second, the strength of evidence for or against the liberalization-convergence hypothesis varies somewhat across control-group type. The data-chosen control groups offer the strongest evidence

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Table 4

Difference in differences in rates of per capita income convergence pre- vs. post-liberalization; liberalizing vs. control countries (the EEC case)^a

Control group	Diff-in-diffs equation	Diff-in-diffs parameter	Number of observations	Number of control countries
World,	(4)	0.001 (0.429)	38	47
All Countries				
OECD,	(4)	0.004 (1.027)	38	11
All Countries				
Europe,	(4)	0.006 (1.354)	38	7
All Countries				
World, ‘Ideal’ Using PCGDP & Trade Data	(4)	-0.013 (-3.414)***	38	6
OECD, ‘Ideal’ Using PCGDP & Trade Data	(4)	-0.005 (-1.311)	38	6
Europe, ‘Ideal’ Using PCGDP & Trade Data	(4)	-0.002 (-0.443)	38	6
World, ‘Ideal’ Using PCGDP & Trade Data	(8)	-0.037 (-0.713)	216	6
OECD, ‘Ideal’ Using PCGDP & Trade Data	(8)	0.017 (0.485)	216	6
Europe, ‘Ideal’ Using PCGDP & Trade Data	(8)	0.006 (0.154)	216	6
Random Trials	(4)	0.001	38	6
Average of All 1000 Trials				
Random Trials, Average of Only Significant Estimates (430 Trials)	(4)	0.002	38	6

^a These convergence rates are parameter estimates of the difference-in-differences regression in Eqs. (4) or (8). The ‘diff-in-diffs parameter’ corresponds to parameter β_4 in Eq. (4) and parameter δ_4 in Eq. (8). *T*-statistics are reported in parentheses for robust standard errors. ***, **, and * denote significance at the 99%, 95%, and 90% levels. Positive (negative) estimates suggest that trade liberalization converges (diverges) income.

against, with the only significant estimate for the EEC case and some of the largest estimates (in absolute value) for the EFTA case. The randomly chosen control groups have two cases with average parameter estimates indicating divergence and two with convergence. But in the two convergence cases a majority (57% and 79%) of estimates did not differ significantly from zero. Third, the overall results seem robust to other ways of generating data-chosen control groups. I also experimented with constructing control groups using just data on per capita income or just data on trade openness. In many instances these alternatives yielded exactly the same or very similar control groups (sometimes due to the small pool of countries to choose from, e.g., when trying to pick from the European pool for the first three cases). For the GATT case it was difficult to rank countries on just

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Table 5

Difference in differences in rates of per capita income convergence pre- vs. post-liberalization; liberalizing vs. control countries (the EFTA case)^a

Control group	Diff-in-Diffs equation	Diff-in-diffs parameter	Number of observations	Number of control countries
World,	(4)	-0.010 (-4.429)***	32	48
All Countries				
OECD,	(4)	-0.008 (-2.804)***	32	11
All Countries				
Europe,	(4)	-0.005 (-1.199)	32	7
All Countries				
World, 'Ideal' Using PCGDP & Trade Data	(4)	-0.015 (-5.160)***	32	7
OECD, 'Ideal' Using PCGDP & Trade Data	(4)	-0.013 (-4.845)***	32	7
Europe, 'Ideal' Using PCGDP & Trade Data	(4)	-0.005 (-1.199)	32	7
World, 'Ideal' Using PCGDP & Trade Data	(8)	-0.017 (-0.728)	224	7
OECD, 'Ideal' Using PCGDP & Trade Data	(8)	-0.015 (-0.693)	224	7
Europe, 'Ideal' Using PCGDP & Trade Data	(8)	0.001 (0.046)	224	7
RandomTrials	(4)	-0.011	32	7
Average of All 1000 Trials				
Random Trials,	(4)	-0.015	32	7
Average of Only Significant Estimates (666 Trials)				

^a There convergence rates are parameter estimates of the difference-in-differences regression in Eq. (4) or (8). The 'diff-in-diffs parameter' corresponds to parameter β_4 in Eq. (4) and parameter δ_4 , in Eq. (8). *T*-statistics are reported in parentheses for robust standard errors. ***, **, and * denote significance at the 99%, 95%, and 90% levels. Positive (negative) estimates suggest that trade liberalization converges (diverges) income.

one dimension because many of the 37 liberalizers shared a large number of the same potential control countries.¹³

¹³As surveyed in Section 3, Ben-David (1993) also analyzes aspects of all four of my cases. He concludes that trade liberalization stimulates income convergence; my analysis indicates this is generally not the case. That said, readers wanting to compare my results with Ben-David's (1993) need to be cautious in a number of ways. For example, Ben-David uses an earlier version of the Penn World Tables and also uses data on per capita income; I use a more recent version and data on per worker income. For the GATT case Ben-David looks at only two signatories, the United States and Canada; I look at all 37. And for the EFTA case Ben-David excludes Austria and focuses more on EFTA's subsequent liberalization with the EEC; I include Austria and treat the EFTA and EEC-EFTA cases separately. Many of these differences reflect different choices about research design. But the overall message of these caveats is that comparisons of this work to Ben-David (1993) need to be made cautiously to understand what factor(s) differ in each case.

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Table 6

Difference in differences in rates of per capita income convergence pre- vs. post-liberalization; liberalizing vs. control countries (the EEC-EFTA case)^a

Control Group	Diff-in-diffs equation	Diff-in-diffs parameter	Number of observations	Number of control countries
World,	(4)	-0.005 (-6.526)***	40	61
All Countries				
OECD,	(4)	-0.007 (-4.873)***	40	11
All Countries				
Europe,	(4)	-0.012 (-4.543)***	40	8
All Countries				
World, ‘Ideal’ Using PCGDP & Trade Data	(4)	0.000 (0.296)	40	13
OECD, ‘Ideal’ Using PCGDP & Trade Data	(4)	-0.007 (-4.873)***	40	11
Europe, ‘Ideal’ Using PCGDP & Trade Data	(4)	-0.012 (-4.543)***	40	8
World, ‘Ideal’ Using PCGDP & Trade Data	(8)	-0.014 (-0.649)	520	13
OECD, ‘Ideal’ Using PCGDP & Trade Data	(8)	-0.029 (-1.481)	480	11
Europe, ‘Ideal’ Using PCGDP & Trade Data	(8)	-0.042 (-1.686)*	420	8
Random Trials	(4)	-0.005	40	13
Average of All 1000 Trials				
Random Trials, Average of Only Significant Estimates (586 Trials)	(4)	-0.008	40	13

^a These convergence rates are parameter estimates of the difference-in-differences regression in Eq. (4) or (8). The ‘diff-in-diffs parameter’ corresponds to parameter β_4 in Eq. (4) and parameter δ_4 in equation (8). T-statistics are reported in parentheses for robust standard errors. ***, **, and * denote significance at the 99%, 95%, and 90% levels. Positive (negative) estimates suggest that trade liberalization converges (diverges) income.

6. Conclusion

Previous empirical studies focusing on trade and income convergence all identify trade’s effect mainly through a single comparison (at most) of two groups of countries. This article has tried to build on these studies by identifying trade liberalization’s effect on convergence by using a difference-in-differences estimation strategy. The main empirical result is that trade liberalization did not foster significant, systematic convergence among liberalizers in any of the four cases analyzed. This result comes from the initial single-difference estimates and the core difference-in-difference estimates using two different convergence measures and a wide range of control groups. In fact, much of the evidence suggests that

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Table 7

Difference in differences in rates of per capita income convergence pre- vs. post-liberalization; liberalizing vs. control countries (the EEC-EFTA case)^a

Control group	Diff-in-diffs equation	Diff-in-diffs parameter	Number of observations	Number of control countries
World,	(4)	0.004 (0.985)	20	89
All Countries				
World, ‘Ideal’ Using PCGDP & Trade Data	(4)	0.007 (1.803)*	20	37
World, ‘Ideal’ Using PCGDP & Trade Data	(8)	0.007 (0.696)	740	37
Random Trials	(4)	0.004	20	37
Average of All 1000 Trials				
Random Trials, Average of Only Significant Estimates (210 Trials)	(4)	0.009	20	37

^a There convergence rates are parameter estimates of the difference-in-differences regression in Eq. (4) or (8). The ‘diff-in-diffs parameter’ corresponds to parameter β_4 in Eq. (4) and parameter δ_4 , in Eq. (8). *T*-statistics are reported in parentheses for robust standard errors. ***, **, and * denote significance at the 99%, 95%, and 90% levels. Positive (negative) estimates suggest that trade liberalization converges (diverges) income.

trade liberalization diverges incomes among liberalizers. This finding is consistent with the theory discussed in Section 2, as liberalization is likely to trigger forces both for convergence and for divergence.

The differences-in-differences approach is by no means flawless. There are some potential limitations particular to it, and some others shared more generally by empirical work on trade and convergence. I briefly highlight four issues as possible directions for future work. First, this approach assumes that trade liberalization arises independently of convergence itself—i.e., that the treatment and the outcome of interest are not jointly determined. This might not be the case. Perhaps countries liberalize trade primarily with countries with whom they’re already converging from various non-trade forces. For example, perhaps domestic support for liberalization requires that very few political-interest groups get hurt from trade. Then liberalization with similar or increasingly similar countries might be preferred if this kind of liberalization changes product prices very little and thus causes very little internal income redistribution via the Stolper–Samuelson process. I have no formal way to rule out stories like this, but simply acknowledge that I treat trade liberalizations as exogenous treatments.

A second issue is that my definition of the post-treatment period does not allow convergence effects from trade liberalization *before* liberalization actually starts. This makes sense for some convergence mechanisms: e.g., FPC arises only as barrier cuts alter product prices. But for other mechanisms timing may be more

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fluid. For example, if liberalization is pre-announced or at least widely anticipated, firms' cross-country investment behavior may respond before cuts actually start. To examine this I replicated my analysis dating the post-liberalization period one or two years before the negotiated onset of tariff cuts. This modification yielded very similar results to those reported here, but this issue of timing might merit future attention.

A third issue is that the value of double-differencing depends a lot on the quality of the control groups. As the paper has emphasized, how best to select these groups is not obvious. It is true that during each case many other countries were also liberalizing trade. But references on recent trade agreements (e.g., Bhagwati and Panagariya, 1996) do not reveal any country groups which were liberalizing the same way as any of the treatment groups—e.g., during the Kennedy Round the rest of the world was not also reducing tariffs by 50%. This suggests there are meaningful differences between treatment and control groups. But future work might be appropriate here.

Finally, a fourth issue future research might explore is the channels through which trade affects income dispersion. Section 2 highlighted several of these channels, and with adequate data such as endowments and factor prices the difference-in-differences methodology could be applied to these questions as well.

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